Galls.

In his suggestive paper on Prof. Weismann's theory, Mr. Mivart says, while alluding to the formation of galls, "It would be interesting to learn how natural selection could have caused this plant to perform actions which, if not self-sacrificing (and there must be some expenditure of energy), are at least so disinterested."

Mr. Mivart here strikes what has always appeared to me one of the most important facts in organic nature with reference to the theory of natural selection. I have always so considered it, because it seems to me the one and only case in the whole range of organic nature where it can be truly said that we have unequivocal evidence of a structure occurring in one species for the exclusive benefit of another.

Moreover, the structure is here a highly elaborate one, entailing not only a drain on the physiological resources of the plant (as Mr. Mivart observes), but also an astonishing amount of morphological specialization. Indeed, the latter point is so astonishing, that when we study the number and variety of gall-formations in different species of plants—all severally adapted to the needs of as many different species of insects, and all presenting more or less elaborate provisions for ministering to such needs—it becomes idle to doubt that, if such cases had occurred elsewhere and with any frequency in organic nature, the theory of natural selection would have been untenable, at all events as a general theory of adaptations and a consequent theory of species. But seeing that the case of galls is unique in the relation which is now before us, it becomes reasonable to attribute the formation of galls to the agency of natural selection, if there be any conceivable manner in which such agency can here be brought to bear.

Now, although it is obvious that natural selection cannot operate upon the plants *directly*, so as to cause them to grow galls for the benefit of insects, I think it is quite possible to suppose that natural selection may operate to this end on the plants *indirectly through the insects*, viz. by always selecting those individual larvæ the character of whose excitatory emanations is such as will best cause the plant to grow the kind of morphological abnormality that is required.

This explanation encounters difficulties in some special cases of gall-formation, which I will not here occupy space by detailing; but as it is the explanation given in a course of lectures which I am at present delivering to the students here, I should like to take the opportunity, which Mr. Mivart's paper affords, of asking whether anybody else has a better explanation to offer.

George J. Romanes.

Edinburgh, November 18.

"Modern Views of Electricity."

Your reviewer (p. 5) takes rather high ground wherefrom to criticize a confessedly popular and expository book; and some of the charges of vagueness—as, for instance, that I do not definitely specify the velocity with which electricity travels in a given current—strike me as rather out of place, seeing that the same charge might be made against the treatise of Clerk-Maxwell. A want of definiteness about the constitution of the ether I must perforce admit; and I can hardly be surprised at your reviewer's want of sympathy with my struggles to convey to non-mathematicians some idea of the tendencies of modern inquiry, when I find that he thinks it "open to question whether attention has not of late years been too much diverted from the condition of the charged bodies in the electric field to that of the medium separating them."

But it is not so clear how, holding this view, he can say that the tentative theory attempted to be explained by me "is in its most important features almost identical with the old two-fluid [action at a distance] theory published by Symmer in 1759"; nevertheless, by taking a few statements from the earlier and introductory portion of my book, and caricaturing them a little, he does manage to make it appear as if the so-called "modern views" were merely a case of reversion to an ancestral type.

However, it is not on these general topics that I break a wholesome rule and reply to a review: it is because I am charged with four or five definitely misleading statements, and it is these I wish to either withdraw or justify.

First, concerning the relation between the Peltier effect and the E.M.F. at a junction. I have argued this matter out fully in the *Philosophical Magazine* for March 1885, p. 269, and have shown that the only "further assumption" needed is this:—*The measure of the E.M.F. at any section of a circuit is the work done per unit electricity conveyed past that section, or,* $dW = QdE$. Until this is disproved I regard it as axiomatic: and, so regarding it, I hold that what I have said about contact E.M.F. is true. My position in the matter is, at all events, perfectly clear and definite, and is fully explained in the *Philosophical Magazine* article referred to, as well as in several others of older date.

Second, as regards tourmaline. I certainly did not *intend* to explain pyro-electricity as due to unilateral conductivity solely, but perhaps my brief statements concerning it on p. 122 might be more cautiously worded so as to avoid any possible misconception.

Third, the "dead-water" argument against electric momentum (p. 103) is not *left* as a valid proof of its non-existence, though it is introduced as at first sight so tending; and all that my critic says against it resolves itself into a question of degree.

The same is true of what he says on the fourth point, concerning Fitzgerald and the Kerr effect; and his assertion that Fitzgerald's deductions do not coincide with the observations of Kerr and Kundt seems to me to convey a much falser impression than my nine-year-old statement (p. 323) to which he objects: "Mr. Fitzgerald, of Dublin, has examined the question mathematically, and has shown that Maxwell's theory would have enabled Dr. Kerr's result to be predicted."

Lastly, my suggested possible account of the Thomson effect (pp. 117, 120, 295), though it does not indeed altogether hold water (as both Prof. Everett and Prof. J. J. Thomson have kindly pointed out to me), breaks down for a reason entirely different from that supposed by your reviewer, who is estimating it only from his own caricature of an ether theory. The real weak point lies in forgetting that the condition required is unequal *impulse*, not simply unequal *force*.

In thus replying to objections raised, I by no means suppose that my critic has made them in any unfriendly spirit. I only feel that he has read the book rather unsympathetically, and (possibly on account of faults in the preface) has regarded it as more scientifically pretentious than its style and object at all warrant. Misleading statements as to matters of fact I have indeed strenuously endeavoured to eschew, and I trust that to very few of them shall I have, in a second edition, to plead guilty.

Oliver J. Lodge.

November 16.

Geometrical Teaching.

Mr. Woodall has called attention to an evil which, even at the present day, is more extensive and persistent than is generally supposed to be the case by those who imagine that "improved methods of geometrical teaching" are making themselves felt.

It is surprising that such a subject as Euclid, which of all subjects perhaps is best calculated to produce in the minds of young persons an exact method of reasoning, should be so badly taught. There can be, I should imagine, only one opinion as to the method of teaching described by Mr. Woodall, viz. that it is decidedly bad; and even worse, that it is perfectly useless.

It is often objected by this class of teachers that young people cannot be brought to appreciate the intricacies and subtleties of Euclid's propositions, and that, in consequence, if they be learnt at all they must be learnt by heart. But is not this a great mistake? My own experience has shown me that young persons *can* be induced to appreciate and take an intelligent interest in Euclid if it be taught intelligently. This demands some little trouble on the part of a teacher, and I suspect that a large proportion of our bad geometrical teaching is due to the disinclination of the teacher to take overmuch trouble in his work, coupled with the fact that it is often very difficult for him to get over the superstition of his own school-days, that a proposition, if it be learnt at all, must be learnt by heart, without any display of intelligent interest.

It does not seem to me to be necessary, at the outset at any rate, in order to improve the teaching, that the ordinary well-known edition of Euclid should be taken to pieces and a new and elaborate arrangement of the propositions made out of the fragments. The effective teaching of Euclid may be conducted upon the old lines, so well known to us in Potts and

© 1889 Nature Publishing Group

Todhunter; but to make it effective our teachers must be possessed of ordinary common-sense. So long as this is absent, all the elaborate and scientifically improved editions of Euclid's "Elements" in the world will not produce the much-to-be-desired change. Let the teacher go through any edition of the first book of Euclid's "Elements" in a common-sense manner with his pupils, and he will find that, instead of the apathy and general disgust exhibited by them when undergoing the ordinary process of Euclidian cram, there will be a general air of brightness, interest, and intelligent appreciation. H.

The Yorkshire College, Leeds, November 25.

A Brilliant Meteor.

WHILE at my observatory to-night, at 9.37 p.m., I saw the largest and brightest meteor I have seen since November 1880. It became visible near ν Eridani, and disappeared near α Leporis. The colour was a bright greenish blue, and the brightness was twice or three times Venus at greatest brilliancy. It cast a distinct shadow. J. COCKBURN.

St. Boswells, N.B., November 23.

STAR DISTANCES.[1]

THE festal offering contributed by Prof. Oudemans to the Pulkowa celebration is an especially appropriate one. The incidents of the long parallax-campaign can scarcely be recapitulated without recalling, in connection with the name of Friedrich Struve, the *quorum pars magna fui* of Æneas. He it was who, in Sir John Herschel's opinion (Memoirs R. Astronomical Society, vol. xii. p. 442), made the first real impression upon the problem by showing that not one of twenty-seven circumpolar stars discussed in 1819–21 could possibly have an annual parallax amounting to half a second of arc. Thenceforward, astronomers knew what they had to expect. Sanguine hopes of meeting comfortably large, and properly periodical residuals among ordinary observations, were checked, if not extinguished. The changes of stellar position reproducing, according to the laws of perspective, the movement of the earth in its orbit, were perceived to be on a scale so minute that their satisfactory disclosure lay, for the moment, beyond the range of what was feasible. Success in the enterprise, it was evident, was conditional upon the employment of more perfect instruments than had heretofore been available with a precision and vigilance of which the very idea was absent from all but a few prescient minds. Sir William Herschel seemed to have anticipated the conjuncture when he declared in 1782 the case to be "by no means desperate," although stellar parallax should fall short of a single second (*Phil. Trans.*, vol. lxxii. p. 83). The memorable "triple event," by which, almost simultaneously, at the Cape, at Königsberg, and at Pulkowa, his confidence was justified, is familiar to all readers of astronomical history. Its significance may be estimated from Bessel's admission that, until the yearly oscillations of 61 Cygni emerged from his measures in 1838, he was completely in the dark as to whether stellar parallax was to be reckoned by tenths or by thousandths of a second (*Astr. Nach.*, No. 385).

The value to students of Prof. Oudemans' synoptical view of what has since then been achieved in this direction can hardly be overstated. Not only does he record every individual result worth considering, but the tabulated particulars enable a fair judgment to be formed as to the value of each. There are, indeed, one or two cases in which a note of warning might with advantage have been added. Thus, Dr. Brünnow's small parallax for 85 Pegasi, to say the least, requires confirmation. A perfect *equability* in the mode of observing is essential in such delicate operations; but the Dunsink astronomer was himself conscious of, and noted with his usual care, a slight change, as the series flowed on, in his habit of "bisecting" the large star (*Dunsink Observations*, vol. ii. p. 38). The distance of this interesting binary system can hence scarcely be regarded as even approximately known.

Still less reliable, though for different reasons, are Johnson's measures of Castor, and Captain Jacob's of α Herculis. The parallax assigned to the latter star of 0″·062 relative to its fifth magnitude companion cannot be other than illusory, since the pair, as evidenced by a small, but well-ascertained common proper motion, are physically connected, and must therefore be at virtually the same distance from the earth.

Forty-nine stars, all save one measured within the last sixty years, are included in Prof Oudemans' list. The exception deserves particular mention. Samuel Molyneux erected at his house in Kew Green in 1725, a zenith sector by Graham, with which he began, in combination with Bradley, a set of observations for parallax on γ Draconis. The same star had, in the previous century, been similarly experimented upon by Robert Hooke with something of a dubious success. The well-known eventual issue of Molyneux's observations was Bradley's discovery of the aberration of light; but they included besides an element of true parallactic change, brought out by Dr. Auwers's discussion in 1869,[1] after it had lain concealed among them for 142 years. The eye and hand must indeed have been faithful thus to record an ebb and flow of change profoundly submerged, at that comparatively remote epoch, in the reigning confusion betwen the real and the apparent places of the heavenly bodies.

A light-journey of sixty-five years (parallax = 0″·05) may be considered the present limit of really measurable stellar distance. Forty of the forty-nine objects so far investigated lie—most of them certainly, a few only probably—within it. Forty stars can thus be located with some definiteness in space—forty among, say, forty millions! The disproportion between our knowledge on the point and our ignorance is so exorbitant that general conclusions seem discredited beforehand, and negative ones at any rate can have no weight whatever. Nevertheless, one remark at least is fully warranted by the evidence.

It is this, that the largest stars are not always those nearest to the earth. For to the narrow category of stars at ascertained distances belong no less than seven invisible to the naked eye, one of them in closer vicinity to us than Sirius, all than Capella, Vega, Arcturus, or Canopus. A cursory view might almost suggest—irrespective of geometrical possibilities—that stellar brightness had nothing whatever to do with remoteness. The legitimate and certain conclusion to be derived from the facts, however, is that the disparities of stellar light-power are enormous. A farthing rushlight is not more insignificant compared with the electric arc than a faint compared with a potent sun. Sirius emits 6400 times as much light as a ninth magnitude star north of Charles's Wain (Argelander-Oeltzen 11,677); our own sun falls nearly as far short of the radiative strength of Arcturus. Inequalities of the same order between the members of revolving systems emphasize this result. Sirius shines like four thousand of its own companions; and the movements of other stars are perhaps swayed by almost totally obscure bodies.

The inference that the apparent lustre of individual stars tells us nothing as regards their distance was already

---

[1] "Uebersicht der in den letzten 60 Jahren ausgeführten Bestimmungen von Fixstern parallaxen." Von J. A. C. Oudemans. Eine Festgabe zum 50jährigen Jubiläum der Sternwarte zu Pulkowa. *Astronomische Nachrichten*, Nos. 2915–16.

[1] *Monatsberichte*, Berlin, 1869. p. 630. The result places γ Draconis at a distance of 35½ light-years, but with a very large "probable error" (parallax = 0″·092 ± 0″·070).

© 1889 Nature Publishing Group